bit of dead furze stem. As he was bending over it, the supposed withered stem moved slightly, and gave him the impression that he was looking at the back of a large viper that had half buried itself in the furze. A still closer scrutiny showed that the semblance of a crooked piece of furze was two fern-owls about three days old.

We have read the book from cover to cover, and have been interested throughout. The author has looked to animate nature for his facts; hence his work possesses the sterling ring which every student of science delights to hear.

Pitt Press Euclid, V.-VI. By H. M. Taylor, M.A. (Cambridge: Univ. Press, 1893.)

WE have previously had occasion to refer to the earlier issues of this series of books on the elements of Euclidian geometry. The present publication is quite up to the standard of the former ones, and contains some important variations from the usual mode of treatment. In dealing with the fifth Book, Mr. Taylor rejects altogether the use of figures, since, as he rightly says, the book is not essentially geometrical. With a general knowledge of proportion as derived from treatises on algebra, a student is sufficiently equipped to follow its applications in Book vi. The numbering of the propositions is somewhat altered owing to the omission of some of the propositions in the Greek text. With regard to Book vi. Mr. Taylor has made some modifications in the treatment of similar figures, and many theorems are more briefly proved by adopting the definition of similar polygons there enumerated. The additional theorems which are inserted have been arranged in series—one, for instance, giving the student a sketch of the theory of transversals, harmonic and anharmonic ranges and pencils, leading up to Pascal's theorem; another of nine propositions, concluding with Gergonne's neat solution of the problem to describe a circle to touch three given circles. The method of inversion, Casey's extension of Ptolemy's theorem, properties of coaxial circles, and some porismatic problems follow next in order, the book concluding with a capital set of exercises and an index for the first six books.

The Out-door World, or Young Collector's Handbook. By W. Furneaux, F.R.G.S. (London: Longmans, 1893.)

A GREAT deal of information useful to the young collector, for whom Mr. Furneaux has prepared this handbook, is to be found in its four hundred pages. There are sixteen coloured plates, some of which are excellent and none bad, and more than five hundred illustrations in the text. Those of birds are somewhat unequal; some indication of relative size would have been helpful. The linnet and the cuckoo are placed side by side, and the former is apparently the larger of the two, while on the opposite page the great tit is considerably bigger than the lark, and rivals the cuckoo in apparent proportions. If the length of the bird had been given in brackets after the name under each figure, it would have prevented misapprehension. We have dipped here and there into the letterpress, and found the information accurate and clearly put.

Worked Examples in Co-ordinate Geometry. (Univ. Corr. Coll. Tutorial Series.) By William Briggs and G. H. Bryan. (London: W. B. Clive and Co., 1893.)

THE examples which are here brought together are intended to serve as a graduated course on the right line and circle, forming thus a useful companion to the book on Co-ordinate Geometry already published by the same authors. The work line is specially designed for the private student, and this is why the problems have been dealt with in such detail, every step in their solution being clearly explained. The examination papers may fairly be taken as good test papers, for the questions seem to have been carefully selected, and the more important ones on book work are not lacking. For those teaching themselves this subject by working out the problems given, a good insight should be obtained, while the references to the author's work on co-ordinate geometry, above referred to, will be found very useful to those possessing that book.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Sir Henry H. Howorth on "Geology in Nubibus."

HAVING given my views on glacial geology in the current issue of the *Fortnightly Review*, to be followed by one dealing at some length with the ice-origin of lake-basins, I should not have thought any reply to Sir Henry Howorth's "Appeal" necessary except for the consideration that my articles may not be seen by many readers of NATURE. And first, I would remark, that the mental attitude which Sir H. Howorth imputes to extreme glacialists I have myself been unable to detect in their writings. In fact, I was under the impression that the "scoffing" and "jeering" was chiefly from the other side; but it seems I was mistaken, and I must apologise for my ignorance. Those who read my articles will see that I make no appeal to "transcendental ice," but judge of its powers and properties by its admitted effects. Sir H. Howorth says that "ice is known to crush under moderate pressure," implying that a glacier a mile or perhaps half a mile thick is impossible. But will he or anyone else tell us what happens to the ice after it is crushed, and the pressure that crushed it is continued and slowly increased? Will it not suffer re-gelation and become denser ice; and if by sudden increase of pressure it is again crushed, will it not by still further pressure again suffer re-gelation? He stops at the first "crushing," as if that were the end of all things so far as a glacier is concerned. All this, however, is beside the question from my point of view. The work of ice on the rocks is as clear as that of palæolithic man on the flints; all the difficulties that may be suggested as to how he lived, or how he shaped the flints do not in the slightest degree affect our conclusion that the palæolithic flint implements are the work of *man;* and there is equally clear evidence that ice *did* march a hundred miles, mostly uphill, from the head of Lake Geneva to Soleure, whatever transcendental qualities it must have possessed to do so.

As to "perhaps the largest and most remarkable collection of rock-basins in the world"—the largest being of 50 acres and the deepest 30 feet deep—I must really decline to occupy your space in showing how simply these may have been produced by ordinary denuding agencies, or in denying that any glacialist, even of "the most extreme and aggressive school," would claim them as proofs of glaciation. As regards the question of Tasmanian glaciation, my last communication to NATURE (Nov. 2) seems to me to render any further observations unnecessary. No doubt the conclusions of the various writers will be fully harmonised by a more complete study of the whole region.

The last point touched on by Sir H. Howorth—whether the advocates of the ice-origin of certain groups of lakes are "extravagant" in their views, following the methods of Aristotle rather than those of Bacon, and founding their beliefs on "purely hypothetical properties of matter and forces of nature"—I will leave to the judgment of those who do me the honour of reading my forthcoming article in the *Fortnightly Review.*

ALFRED R. WALLACE.

The Erosion of Rock-Basins.

MR. T. D. LATOUCHE'S letter (page 39) is very interesting as a more than usually independent contribution (for the reason given therein) to the interesting question of glacial

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erosion, and as showing how similar (allowing for the difference in size) are the phenomena of the Himalayan and the Alpine glaciers. But I think that moulins, as a rule, are not likely to be very important agents in the formation of the rock-basins in which lakelets and tarns are often lodged. So far as my experience goes, the range over which the moulin-torrents can act is very restricted; for the crevasse, which gives the opportunity to the water, is generally formed very nearly at the same part of the glacier. Thus after the moulin has travelled for a very short distance down the glacier, a new crevasse opens out behind it and cuts off the torrent. I have frequently seen four or five dry shafts in advance of the working moulin. The lateral range also of the moulin must be small. Hence I think that the giant's-kettle (as is usually supposed) more accurately represents the ordinary product of a moulin. An excellent illustration is afforded by the well-known "glacier-garden" at Lucerne. I think, also, that the rock-basins, of which we speak, are more commonly found in situations where moulins would not be numerous or large, viz. in cwms and corries. It is, however, true that in certain undulating rock districts, as parts of Scandinavia and the Scotch Highlands, lakelets are common. The form of these, however, does not appear to bear much relation to the hollow produced by a moulin. So that I doubt whether we can regard a moulin as an agent of primary importance in the production of an ordinary rock-basin, though it may sometimes be a minor contributory. As I have more than once discussed the question of the probable cause of the formation of tarns as well as of large lake-basins, it is needless to repeat what has appeared in print. T. G. BONNEY.

23 Denning Road, N.W., November 13.

"The Zoological Record."

IN your Notes for October 26, on p. 621, you follow the Editor of the *Zoological Record* in suggesting that, under the present financial conditions, palæontology should be removed from the volume issued by the Zoological Society, and provided for by the palæontologists themselves. Against such retrogression we desire to protest. "Everyone knows," as you say, "that an incomplete record is of very little use"; and how absurdly incomplete a record would be that took no account of palæontology! The objectors probably spring mostly from the ranks of systematic zoologists. We will deal with them on their own ground. The systematic position of *Limulus* has long been a vexed question, which no one can attempt to solve without consulting the work of Malcolm Laurie on the fossil Eurypterids. The classification of the Crinoids has troubled zoologists since the days of Johannes Müller; but neither he nor anyone ever dreamed of settling it without reference to palæontology. Students of recent Bryozoa will not be grateful to those who keep them in ignorance of J. W. Gregory's lately published work on the Bryozoa of the early Tertiary rocks. And so we might go on *ad infinitum*. Another argument that may affect the systematists is that if they reject all names of fossil genera and species from the record, they will have no means of knowing whether the new names they may wish to propose have been used before or not. It is even possible that some of them may unwittingly describe as new forms already described by some unknown palæontologist. It is hardly necessary to remind the morphologists, embryologists, and zoögeographers of the help that they constantly receive from the palæontologists; they, at least, will not wish to have the record made incomplete.

It is suggested that every branch of science should have a record, and that palæontologists should undertake the compilation of a separate one. This would as good as double the work, both for recorders and students. What we have said above shows that palæontology is not a separate science. Zoologists and palæontologists ought to be the same people, and when they have strength enough they are so, as the names of Cuvier, Owen, and Huxley sufficiently testify. The palæontological recorder would still have to work through the writings of the zoologists, while even the pure neontologists would have perpetually to refer to the palæontological record.

What is really wanted is to complete the *Zoological Record*, not to make it incomplete—to go forward, not backward. It is admitted that some of the recorders do tackle the palæontological literature. Why should not all? If a group is too large for one man, then give it to two, and if a second man cannot be got to work on half-pay, then double the pay.

To prevent the record becoming too big, make it merely an index, and cut out the abstracts, which are rarely correct. If more money is wanted, appeal to other societies which might naturally be supposed interested in the work. It is unfair that a single society should bear the burden of a work that is of value to all, and one can hardly suppose that it would refuse kindly offers of help. We believe, indeed, that the only reason why some of the recorders abstain for the present from the palæontological work is because they feel that part, at least, of the expense ought to be borne by the society more directly interested.

R. I. POCOCK,
F. A. BATHER,
B. B. WOODWARD.

British Museum (Nat. Hist.), October 30.

Recognition Marks.

A QUESTION in natural history has occurred to me, which, I think, might with advantage be discussed in your columns.

It is usual to account for the white tail of the rabbit (*Lepus cuniculus*) by saying that it is useful as a danger signal to others of the species. Wallace, in his "Darwinism," speaking of rabbits, says that "the white upturned tails of those in front serve as guides and signals to those more remote from home."

Now, there appear to me to be two objections to this theory. The first is that the tail of the hare (*Lepus timidus*) is also white, and is turned up in precisely the same manner when running; but it is obvious, from the habits of this animal, that in its case it would be quite unnecessaary for such a purpose.

And in the second place, if this were so, how could it have been produced by evolution? The object of the white tail is said to be to assist other rabbits to escape, not the possessor of the white tail itself. But the principle of evolution is the survival of the animal fittest to preserve its own life, not of the fittest to preserve the lives of others of the same species.

G. J. MACGILLIVRAY.

3 Belford Park, Edinburgh, November 6.

MR. MACGILLIVRAY has failed to grasp the principle of natural selection when he thinks that it cannot produce a character useful to other animals of the same species. The action of natural selection is to preserve the *species*, as well as each individual separately; and, consequently, every character useful to the species as a whole would be preserved. This is obvious when we consider such characters as nest-building in birds, and milk-secretion in mammals, which do not benefit the individual possessors, but their offspring; and the same principle applies to every character which is mutually useful to individuals of the same species, as are what I have termed "recognition characters." Neither can I admit that the habits of the hare render the white upturned tail "quite unnecessary." The hare is a nocturnal feeder, and a mark which readily distinguishes a friend from an enemy, and enables the young during their short period of infancy to keep within sight of the mother, must be of considerable importance.

ALFRED R. WALLACE.

Correlation of Solar and Magnetic Phenomena.

IN writing on this subject (NATURE, vol. xlix. p. 30), to save space I omitted to refer to one other case of presumed connection. But as such omission might be misunderstood, may I here briefly allude to it? M. Trouvelot, on June 17, 1891, observed changes going on in connection with a luminous appearance near the western limb of the sun, such as he had not before seen. But the magnetic movement was in this case insignificant (see *The Observatory*, vol. xiv. pp. 326-328). The same reasoning as before may be applied. If the smaller magnetic motions do really directly depend on solar changes of so marked a character, how does it happen that many greater recorded magnetic movements remain without corresponding solar change having been seen? It is a very interesting, indeed critical point, but much more information is necessary to prove that such close connection really exists.

The appearance was seen by Trouvelot near the sun's limb. There is a significant sentence ending a letter from the Rev. Walter Sidgreaves, of Stonyhurst (*The Observatory*, vol. xiv. page 326), as follows:—"But there are no indications of magnetic disturbance accompanying the solar eruptions seen through the spectroscope. Even the brilliant display on the western limb, of the 10th [September 10, 1891], has left nothing that

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